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The world in axioms: an interview with Patrick Suppes

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INTERVIEW

The world in axioms: an interview with Patrick Suppes

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I met Patrick Suppes on 18 March 2014 in his office at the Stanford's *Center for Study of Language and Information*. Until his passing away in November of that same year, he was still actively engaged in research at the *Suppes Brain Research Lab*, a laboratory that he had founded in the 1990s and where he investigated questions around language and human cognition. We had arranged the interview long before our meeting, but when he entered the center, it became clear how busy he still was: responsibilities from various sides before the interview; phone calls and signature requests in between the interview. He had already reached the age of 92, but seemed more energetic than many colleagues half a century younger. This energy was also what struck me during the interview. I had probably not met somebody as broadly literate and eloquently articulated as Suppes before. That itself might not mean much. And it confirmed in a way what I had repeatedly read and heard about him from his former colleagues and his admirers who had interacted with and had followed him throughout his career. But what means much, I think, is that I was able to confirm this observation when he had reached his 90s.

Patrick Suppes had received his B.S. in meteorology at the University of Chicago in 1943 and his Ph.D. in philosophy at Columbia University under the supervision of Ernest Nagel in 1950. In that same year, he was appointed assistant and subsequently associate professor of philosophy at Stanford University, where he first came into context with Tarski's logic and set-theoretical models. In 1959, Suppes was appointed professor for philosophy as well as director of the *Institute for Mathematical Studies* in the Social Sciences at Stanford University. He had stayed at Stanford ever since, and he never really retired.

I conducted the interview for a book project entitled *Conversations on Rational Choice Theory*, which aims at bringing together the views of scholars who were and still are engaged with developing and applying various approaches of rational decision-making within and beyond economics. An interview with Suppes was a natural choice for such a project and for multiple reasons. One reason was that throughout his life, Suppes had been one of the strongest defenders of the axiomatic method in his work, a major ingredient of modern theories of rational choice. He had been deeply involved in working on the foundations of psychology and was one of the pioneers in using formal mathematical tools to approach human decision-making when they had been introduced in the 1950s and 1960s into the social and behavioral sciences. Beyond that, Suppes

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5 had worked on problems concerning the foundations of physics, on the theory of measurement, the measurement of utility and subjective probability in uncertain situations, and on learning theory. His research covered topics in philosophy of science, such as on problems around causation. And he made pioneering contributions to computerized learning, the development and testing of general learning theory, and the semantics and
10 syntax of natural language in philosophy of language. As such, he was a polymath, and as such, he will always be remembered.

Catherine Herfeld: Professor Suppes, what do you take rational choice theory to be?

15 Patrick Suppes: It is an axiomatic theory of choice yet it could also be non-axiomatic. But I think the axiomatic theory plays a particular role in isolating things in cases where it is hopeless to verbally theorize about without contradiction. So I think it should be done axiomatically, if one can. At the same time, I think that although it is as such a highly mathematical theory, we see the right continued development of it in recent years. It has become an ever-richer empirical subject, in terms of bringing in
20 additional concepts.

C: What do you think it is that makes it a *theory* of rational choice? Is it the set of rationality axioms?

25 S: Making a theory of rationality can be done axiomatically but does not have to. However, despite the development towards a more empirical theory, I ultimately think that the right way to do it is axiomatically, if one can. I think the axiomatic theory plays a particular role in isolating things, where it is said that it is hopeless to theorize, where we encounter contradictions.

C: What makes rational choice theory to be a theory of *rational* choice?

30 S: I am very wary of the use of the word rational. I talk about choice theory. Delete rational!

C: How did you become interested in choice theories?

35 S: I worked very closely with David Blackwell and Meyer Girshick. Herman Rubin and I studied their book *The Theory of Games and Statistical Decisions* very carefully. That was an influential book on theory of games and decisions. And it was really important for me.

C: ~~How~~ did you first ~~get into~~ ~~met with~~ the axiomatic method?

40 I think I had two forces in my life at work that influenced my axiomatic view. One was working on the book by Blackwell and Girshick in particular. The other was Alfred Tarski, with whom I became acquainted in Berkeley and during the time I studied game theory. So I met Tarski, but that was also because J.C.C. McKenzie was here. He must have been on the order of 10 or 12 years older than me. And he knew Tarski from back in the 1930s and so, on account of him, I met Tarski. We had very close interaction with Berkeley, with a lot of the Berkeley students, and they had a completely axiomatic view of the world was the dominant, foundationally at that time.

C: You mention in your autobiography that you were organizing an informal seminar already during your graduate studies in philosophy at Columbia University where you discussed the *Theory of Games and Economic Decisions* by John von Neumann and Oskar Morgenstern. How come that you were already interested in game theory before you came to Stanford University?

S: Yes, in 1947–1948 we read the theory of games with some graduate students. My advisor in philosophy was Ernest Nagel. My history is complicated because of interruptions due to the war. My interests had been originally in physics, and so I came to philosophy only after the war on the Servicemen's Readjustment Act, i.e. G.I. Bill of rights. I continued to work while the government supported me as a graduate student in philosophy. At Ernest's encouragement, I took both graduate courses in physics and mathematics, and from both of the directions, particularly the mathematics, as far as axiomatics goes, there was somebody who was quite prominent, Samuel Eilenberg. He was a Polish mathematician, a topologist, who exemplified a very abstract view. He gave beautiful lectures on group theory, which were very abstract, though, much to the disgust of the physicists, who expected from group theory something more practical from their standpoint. That was in 1947 and 1948.

C: Back then, the axiomatic method was important mostly in mathematics and physics. What did you, as a philosopher, expect from the axiomatic method at that time?

S: I became interested in it during a time when contemporary mathematics was just making a transition to a more or less full adoption of a set-theoretical axiomatic viewpoint about mathematics and so there is a lot conceptual lens throwing in and old-fashioned analysts did not like it. When I came to Stanford for example in 1950, we had some superb European analysts who were refugees. They were trained mathematicians of an older generation. They had been raised on analysis, according to which there was only one real system for them. They did not think of it axiomatically. Real and complex analysis, this was the truth for them. And they were remarkable. They did beautiful work, but without an emphasis on axiomatics. The point is that it was complicated. At the same time, there was this corresponding movement in the United States that was really emphasized by a number of people, and I got early support on account of it, to emphasize people with mathematical background to work in the social sciences and behavioral sciences. And, for various reasons, a lot of our work was axiomatic in character.

C: What were the reasons for taking this focus?

S: That is a good question. This was in the late 1940s and early 1950s. I think it reflected the fact that, preceding that, there had been this big turn to axiomatic methods in mathematics itself. If you had looked at mathematics in America before or during the war in, say 1937, this would not have been that way. Mathematics itself was not done so axiomatically. So the transition had been happening in mathematics. It was accelerated by people like Eilenberg and other scholars at Columbia, but particularly Eilenberg. He was a great influence there. And that sort of spilled. That's surely some off the cuff insights into this history that are probably correct. Ken Arrow was involved in this early history before I was.

C: The axiomatic method eventually became adopted in psychology and the social sciences. This is interesting because initially it did not naturally lend itself to address many of the core problems in those disciplines. But around the 1950s, scholars from various disciplines – not only in economics but also in psychology and philosophy – started to use the axiomatic method.

S: That is true. A lot of people did not like it. But then Ken Arrow's dissertation had some examples of showing the use of the axiomatic method in economics, showing negatively to how certain things are impossible. I think similar things happened in the theory of preference, maybe related to what Arrow did but now focused on the individual. And there was a lot of dispute about subjective probability and about utility theory. Such disputes – similar to much earlier disputes like Zermelo-Fraenkel set theory – are responses to what seemed to be contradictions. In other words, the use of axiomatics is inspired by contradictions.

The axiomatic method has this old tradition. It was probably really first introduced in an important mathematical way by the ancient Greeks. The ancient Greeks were responding to the very first problem. More precisely, there were two problems together, namely that the square root of 2 and pi are rational numbers. And there was huge dispute about this in the fourth or fifth century B.C. It is really in Greek mathematics where the axiomatic method did begin. It was so unbelievably sophisticated already then. There was nothing corresponding to this mathematical sophistication of the Greeks. Remember the problem was to prove consistency, so you had already both the axioms and the problem of consistency.

So, to see the origins of these problems, you have to look into the past. In the case of set theory, it goes back to Zermelo-Fraenkel set theory. In 1900, David Hilbert, at the international congress of mathematics, gave a list of famous open problems. Problem number one was to prove the continuum hypothesis. And of course he believed it could be proved. After the earlier work from Ernst Zermelo and Abraham Fraenkel, including Kurt Gödel's work, the most striking piece of work, was Paul Cohen's proof of the independence of the axiom of choice from Zermelo-Fraenkel set theory with the axiom of choice. What that showed was, in some sense, the limitations of our thinking about what should be the foundations of mathematics. It showed that it is not just routine but rather that once we have done that, then there are real problems with how to think about what is the total foundation.

C: Together with Kurt Gödel's two incompleteness theorems and also John von Neumann's confrontation with the limitations of axiomatization in mathematics, this history did not necessarily make the axiomatic method attractive. Why did scholars begin to use the axiomatic method extensively in the social sciences?

S: Well, you have to remember something else. We had these results but historically, scholars had made this huge effort in others parts of science, but especially mathematics, to provide an axiomatic foundation. This urge, generated way back with the Greeks, came from the belief that the proper way to do things in mathematics and the sciences was in that way. Ptolemy's astronomy, written in about 100 AD is in some sense a more rigorous book about astronomy, including observations, than a lot of work that came much later by astronomers in the twentieth-century.

Ptolemy had written in this very rigorous, Euclidean fashion. And you see they also have a problem, like the continuum hypothesis. Ptolemy's astronomy was based upon

the hypothesis, starting with Eudoxos, that all the orbits are circular, or compositions of circles. They discovered quite early that simple circles would not work and that you needed the composition of circles. Yet, these circles had a very strong restriction as part of their classical theory of axiomatic foundations of astronomy. The angular velocity must be constant, of the rotation of the Earth. They found already in Ptolemy's time that this would not work, unless you had many more convolutions of circles, which they did not want. So they would add either epicycles or epicenters. An epicycle is where you have an orbit and now you introduce a small [perturbation of an] orbit on the circumference. An epicenter is where you dislocate the center, which was a very big move, from being at the center of the Earth. So epicenter was to move the center off in order to fit the trajectory of a particular planet.

Now of course it was Apoloneus who proved the equivalence of these two mathematically, which is quite surprising. They have many different mathematical descriptions. Any orbit that had an epicenter representation had also an epicycle and conversely. So already there you see that it was considered very important to understand, starting from a very clear axiomatic basis, how the universe was working. And the impulse to that came out of this Greek tradition. It did not exist in Babylonia. The Babylonians were very good at computing, but at least all the stone tablets that we have read so far do not show anything comparable at all to Greek thought in this axiomatic way.

I think this history is very important to understand this history of axiomatics. And what is interesting is that it was not just pure mathematics. The application of mathematics to the motion of the heavens was *the* most successful empirical example in the ancient world. There were other good examples; some work in mechanics for example by people like for example Archimedes. But that was above all the most striking example.

C: So was the use of the axiomatic method primarily an attempt to make the social sciences more scientific in the 1950s?

S: Sure, absolutely. But I think that there was another impulse, which had been the same one that dominated Greek early thought, namely to address these paradoxes that we were talking about. Some of those paradoxes have a long history, like for example the gambler's fallacy. So, there has also been a long history regarding the work on decision-making and betting. And particularly here, axiomatization was an attempt to get rid of those paradoxes. One basic impulse is, when you have a paradox that seems to give you a contradiction in your intuitive way of thinking, try to axiomatize what you think the true theory is. Because a true theory that is consistent cannot have paradoxes.

C: Axiomatization seems to have had a different effect in the behavioral and social sciences. Take the kind of paradoxes that Leonard Savage encountered in the early 1950's with the axioms of his subjective expected utility theory or the Allais paradox. Those examples seem to be different from those you have in mind because they were provoked from the empirical weakness of those axioms. Savage himself failed to conform to his axioms.

S: And he then felt the axioms needed modification.

C: Those paradoxes did not arise from internal inconsistency of the system but rather from failing to conform to empirical evidence. And one response of Savage

to Maurice Allais was that indeed those axioms were not useful for empirical, but only for normative purposes.

S: Well of course that is a different kind of response. And Savage wrote his book in 1954. That was in the early days. The literature became huge on this, particularly in economics and in psychology. Even today, there is a new discussion. And one attempt of a new discussion was to try to eliminate problems by giving an appropriate axiomatic foundation. In many of the cases, consistency is much more a problem in the minds of mathematicians than it is of economists. But although Savage just reinterpreted his theory, there was still some concern to show that theories were consistent.

C: That's true. Arrow's proof of inconsistency was, according to Arrow himself, a reason for why his dissertation was very much regarded in some areas of economics as something profound.

S: I think people also realized that there was a huge amount of cluttered normative thinking. And one of the ways to show that this does not all fit together well is to show that they are inconsistent, and then the question is what kind of positive consistent theory can you construct, and the natural way of doing that is to think axiomatically. That is, the axiomatic method is a natural response to the existence of inconsistencies in intuitive thinking. I think that is an important idea. Do you agree with that or not?

C: One would probably agree with that if committed to a particular image of science

S: This is theoretical thinking about science. Maybe this thinking can be also empirical because empirical data are also used to show that classical theories do not work.

C: What is the usefulness of mathematics in science beyond guaranteeing the consistency of the theories?

S: Mathematics, and also statistics, is most useful when it can also be used for predictive purposes. You see the history of axiomatics in mathematics turned out to be much more convoluted than it had been thought. The results of Gödel and Cohen and other people show that you cannot prove consistency unless using a theory that is more powerful. You could not prove ordinary mathematics, let's say classical analysis, as being consistent without using a theory that was still more powerful, and whose proof of consistency was even harder than the theory you're using it to prove. But you could not prove with a system of the same strength that it was consistent. That's a great result.

C: Isn't Arrow's impossibility theorem a striking result?

S: That is a much more specialized theorem. I mean big time theorems in mathematics as a whole. But, yes, it is an important result.

C: Apart from the fact that it allows for guaranteeing the consistency of the system, what do you consider the empirical usefulness of such a theoretical undertaking?

S: It takes some of the wind out of the sails of overblown theories, and you cut theory back to having a more empirical character. For example, if you want to be very finitistic, you can prove consistency. Something you can do is to discuss the consistency of

arithmetic, if you consider no number larger than some fixed natural number. There was a discussion about these aspects, about finitistic theories of utility, in the 1950's. The problem with Savage's book of 1954 was that he used infinite sets of states, etc. which clashes with the real world. It was written in a specific way, in a kind of classical mathematical style. One kind of response was that we can be finitistic about this. Now, I think economics has not stayed that way in general at all. In fact it is very unusual to have highly constructive finitistic theories in economics. They have gone back to depending mainly on classical analysis.

C: Recently economists have made new attempts to improve economic theories of behavior by taking psychological and neuroscientific results more seriously.

S: Sure, and that's the direction of complexity.

C: One hope might also be that psychological theories could replace axiomatic foundations at some point.

S: Well I think the following point isn't always made explicit and it has its own weaknesses, but if you make everything sufficiently finitistic, then, in principle, you can just give that. Axiomatics can be given in a kind of clear but uninteresting way. So you have a finite set of axioms; the models are all finite. But, some of those models will lead to very complicated sets of axioms. And of course what people want is something that is more reduced than that. So what may happen, if you abandon trying to give relatively simple axiomatic theories, is that the theory now no longer has a clear formulation. Much of economics is still that way. All you have to do is turn on the TV to see and to listen to amateur economics discussed endlessly. And so what happens is that you no longer have a budding science; you have something much weaker.

C: But does this not also depend on how axioms are justified? Your own work in psychology for example is highly formal in parts, yet it was significantly inspired by your empirical work.

S: Yes! Because my thrust was to get at what really the basic assumptions are in each case, in order to clarify what follows from them. Many of the people in psychology who wrote the theories didn't know any mathematics or very little.

C: Did economists just get it wrong with their axiom systems so far? Should they have taken other sets of axioms, axioms that are more empirically inspired?

S: Remember you have something going on in economics that isn't mentioned enough on this side of discussion, and that is econometrics. So there was a strong statistical econometric tradition going back into the nineteenth-century, at least. And there was not a strong interaction between these two. I do not think that econometrics is mentioned once in Savage's book, for example.

In the neoclassical tradition of economics, there are so many theoretical papers that don't come even close to data of any kind. And that's very important, because I think that the big difference between economists and psychologists is that while economists are better theoreticians, psychologists are much better at having found good data to try to support theories. In Kenneth Arrow's thesis, for example, there is almost no flow of experiments that directly follow from that work. What flows from it is a body of theoretical work.

C: At which level could data enter the picture in such a theoretical framework?

5 S: What actually happened in physics is a good example. Newton's work was not purely theoretical. Rather, there was a continual interaction between theoretical and empirical work, i.e. between the data and the correct estimation of the parameters in the theory. That is the glory of celestial mechanics; in the eighteenth-century, particularly at the end of the 17th, it was recognized that you had this very powerful interaction between theory and data. It wasn't even about experiments because what they used
10 was astronomical data. But, sure that doesn't really occur too often.

C: Some economists tried when they considered classical mechanics as their role model for a very long time and considered utility theory as a useful starting point for economic theorizing as it approximated human behavior in the market sphere.

15 S: But name an economist who was serious about that. They weren't serious about it. That's one thing that's very hard work.

C: William Stanley Jevons or Vilfredo Pareto?

20 S: Well Jevons and Pareto were too late. I'm talking about the earlier period. Newton's famous papers on this were written at the end of the seventeenth-century, and they had a big impact on scientific work in the eighteenth-century.

C: Economics is a much younger science than physics. What some economists tried was looking at the successful theories in physics and then formulating an analogous theoretical, deductive framework. They believed that they might be able to arrive at a theory that could be based on a small set of behavioral laws that could ultimately be formulated in mathematical terms.

25 S: I think it is nonsense. It is really romantic nonsense.

C: Formulating a general theory of human behavior and social interaction seems to be just a huge challenge ...

30 S: It is the same challenge in physics. I remember a famous lecture I heard in the late 1940s or early 1950s by a prominent mathematical physicist. He said: we owe our students an apology. We have taught them as if physics is really simple, when in fact, we as physicists can only solve the simplest cases completely. So I like to say that celestial mechanics is really a great science, for $n = 1$ it's fine, for $n = 2$ you can do very well, but go to $n = 3$ particles, and you're stuck. The behavior you cannot give a complete
35 account of. And that isn't very far into it. And if you go to relativistic mechanics, classical mechanics now formulated for relativistic purposes, $n = 2$ is not solvable in closed form, so it's only $n = 1$. Quantum mechanics is completely weak in studying the interaction of atoms or particles or whatever it may be.

40 **C: In your autobiography, you state that your knowledge of meteorology had stood you "in good stead throughout the years in refuting arguments that attempt to draw some sharp distinction between the precision and perfection of the physical sciences and the vagueness and imprecision of the social sciences. Meteorology is in theory a part of physics, but in practice more like economics, especially in the handling of a vast flow of nonexperimental data." This sounds very much like**

what Kenneth Arrow said, when talking about his disappointments with physics and its inexactness as a science when he worked as a meteorologist. You were both saying that you were disappointed by physics, because it turned out that physics is not the exact science that you thought it was. If neither physics nor economics have achieved to be exact sciences, what does that imply – conceptually and methodologically – for how those sciences should be practices?

S: Yes, Ken and I discuss that regularly, we were both meteorologists in World War II. My view that I've come to gradually late in life, is that science is mainly extremely fragmentary in character as opposed to the idea of it being a well-organized thorough body of knowledge that explains to us mainly how the universe works. Like experience, it is very fragmentary. The cases can be studied thoroughly are extremely limited.

C: Some people might argue that this would go against one of the most important motivations for axiomatics.

S: No, not at all. At least what you've got you can study axiomatically. What you haven't got, you can't study at all. In that sense, axiomatics helped clarify. Only if you start with a very clear basis you can come to see that the situation is in fact hopeless. If you don't have a strong firm basis, people can think mistakenly. If we think of the right function here, we're going to be able to get our way through this. Whereas starting from a clear axiomatic basis, to prove incompleteness or insolubility, or the impossibility of a closed form solution to a differential equation, is a great triumph of the human intellect, to understand that the world is complicated. And only if you had a good foundation do you believe those proofs being correct. I mean that's probably one of the best examples, that very familiar differential equations, both ordinary and partial, do not have closed form solutions.

C: That is interesting, because for example your work on the foundations of measurement was an attempt of trying to lay the foundations of measurement for all the sciences.

S: My own view changed; my earlier views, when I was younger, were wrong. When one is younger, one is too naïve. I was, and most people are, too naïve about the possibility of having genuine wide-ranging semi-complete solutions to things. That is much too optimistic. And it has taken a long time to realize the fact that what we can do with mathematics about natural phenomena is much more limited than was originally thought. Meteorology is a good help to see that. Everybody talks about the weather but nobody can thoroughly understand it scientifically in terms of what you would like to have. You would like being able to predict the weather two weeks from now. That is an insight that has come late to me.

C: In your work in the 1950's with Donald Davidson and Sidney Siegel you did experiments where you showed that the results of expected utility theory were not that satisfying.

S: We already showed that in these finitistic cases, it didn't work out the way you liked.

C: Did this already lead you to doubts about a science of rationality?

S: Absolutely. This was in the special case for rationality. But now I am talking about a wider generalization of mine, that this intuitive science in general is fragmentary in character. Almost no problem of any complexity can be solved completely in science. And we have illusions of grandeur as to what we can do theoretically.

C: Was this early work driven by the general idea that we can find the general axioms of the social sciences?

S: It was a mistake to think we're going to do that, finding general axioms of the social sciences. The discussions of Savage and others turned out to be dominated by a very naïve view about human behavior. And I think some of the things that I probably wrote then, I would now consider naïve, even though I was already on the side of the position that it is hard to solve things, hard to find cases where utility theory really works.

C: One could argue also that this kind of work has nevertheless certain usefulness. First, it appears to be very challenging in general to find a general theory of human behavior and utility theory had some specific features that were useful at least for modeling purposes. Second, its usefulness might also depend on the problem at hand, which varies across disciplines. The theory might suit for one purpose better than for another.

S: Well measurement was one thing. I think theoretically I would consider my work on stochastic learning models as more important in psychology. But certainly measurement has been important in psychology. It had its important place in economics as part of a general discussion. I am in my reflective later years now and I can say that. But already back then, I think Duncan Luce pushed us more in the direction of mathematics and the foundations of measurement than I thought was a good idea. I can remember our discussion about those issues and about how they should be handled. I thought we should deal more with finitistic examples, the statistics of real measurement and I can remember Dave Krantz saying that the trouble would be that it would going to be too hard to do thoroughly. My own criticism of the foundations of measurement was that too much time was spent on what I would consider as being not very interesting, reasonably elementary, mathematics. It would have been much better to have given a much more thorough treatment of finite models with error, and to have analyzed the theory of error, the statistics of error very thoroughly because this was something psychologists themselves did not and don't know and understand too well. Yet, we didn't lead the way we should've, that's my personal view. Upon reflection, this is my criticism of that work in which I partly joined in and as such it is criticism of myself.

C: What did you think of axiomatic work in economics such as Gerard Debreu's *Theory of Value*?

S: I have the same criticism of Debreu, and I knew him quite well. He never really applied a theory in detail to any complicated data.

C: Why do you think economists have not been as much concerned about applying their theories to complicated data as they were with developing complicated theories?

S: I think that this is an interesting historical question. Why have there been these two rather separate traditions in economics? What is interesting about it is that the people who have been interested in data are often quite sophisticated mathematically, because they have a background in mathematical statistics. That's good but it's a different background, you know. The development of the theory of mathematical statistics took place in the 1940s and 1950s. Many of those people, Kenneth Arrow for example with lots of mathematical background, were also working in economics, I mean, for example, the whole theory of IID random variables, and the asymptotic theory of that, which has its own application, it's also beautiful mathematically, but that was really something very different than, say, neo-classical theory.

For us, an important book such as for example J.L. Doob's book *Stochastic Processes* was only published in the 1950's. One of the things we wanted to understand was the clarification of probability, and we were not so much concerned with statistics. Initially, the Russians had best done this. Take Andrei Kolomogoroy's famous work in German, which had been published earlier in Russian, was only 1933. We were very late, and the Russians were determined to show that probability was very much a, should be a standard mathematical theory. So there's no question, you see, that the Russians were numero uno in the first half of the twentieth-century in probability theory. There were good people elsewhere, but the Russians dominated the results. And what they were showing, that there was a strong mathematical theory required to solve standard conceptual problems in probability theory.

Too many people ignore the Russians. The Russian tradition was not really statistical, but it was probability all the way down. They were late to really digging fully into modern statistical procedure, with the random variable set up, and data, etc. that is, that whole modern set up of mathematical statistics. And yet they proved the largest body of important theorems. The largest body was clearly proved by the Russians, starting with Alexandre Lyapunov and that is all written down in Andrey's Kolmogorov's work. And their work is also an interesting example of axiomatic theory. Kolmogorov is very clear about being axiomatic. He says it's like Hilbert's axioms, which he was wrong there. Because he doesn't start with qualitative axioms and derive a quantitative representation, but clearly it is axiomatically written, and he's very clear about writing it that way. And that was a clarifying thing already in 1933, which is pretty early in this story, that's like roughly 23 years before Kenneth Arrow's dissertation was published. It took two decades. And that was a very important influence in probability theory.

I mention probability theory for an important reason because it been so important in the social and behavioral sciences as a mathematical apparatus. But the firm foundations of probability theory don't date much before 1933. Laplace had done interesting work and Abraham De Moivre but really getting the foundations straight, axiomatically, came quite late, which is an interesting story in itself.

C: In economics, there was J.M. Keynes wrote a treatise on probability ...

S: Yes, but it is terrible. It's a mess. It's particularly a mess, given when it was written and given that the Russian had done before. And on the scientific side, Keynes's book on probability never had a big impact, and rightly so.

C: What makes an axiomatic theory a good theory?

S: I think Isaac Newton's work was good. Newton really clarified in some deep way how to think about the foundations of mechanics, which was not so clearly done

before. That doesn't mean there aren't predecessors. But Newton was the one who really crystallized it. And what's important about this is that he wasn't just crystallizing the foundations, but he was doing something important with it, namely to develop an axiomatic theory of gravitational interaction, that is, the most pervasive and important and mystifying approach of gravity. We still don't quite know how we want to explain gravity, but we certainly know that gravity is there. And Newton said he had no place for hypotheses. He clarified, without explaining it in some philosophical way, what is gravity. He clarified what the laws are, the primary law, of attraction. And that was a very important clarification. Descartes' theory of gravitation for example was hopeless. It was a field theory, so there wouldn't be action at a distance, but of course it was physically wrong and it was shown decisively so by Newton and others. Two generations after Newton they were still using Descartes in the introductory course of physics at Cambridge University. So the world moves slowly.

C: Was the work of von Neumann and Morgenstern on formulating a theory of human *interaction* comparable in its significance for the social sciences?

S: Some people think that and I think actually, in the terms of the methodological effect it has had, there is some sense that John von Neumann's big treatise on the theory of games had a big impact, in introducing a way of thinking formally about multi-person interactions.

C: In your paper *General Remarks on the Study of Preference*, you argue that human behavior is hugely complex, which limits the empirical value of any simple and general psychological theory of decision-making. At the same time, you criticize the heavy focus in economics on optimization, which – one could argue – comes however with the benefit of being able to use sophisticated mathematics to formulate economic problems in a precise manner. How do you think should the trade off between an empirically more adequate and useful theory of human behavior and a simple mathematical theory be made?

S: I think, of course, that the empirical side wins that battle. The world is not going to change in some drastic way to satisfy some simple theory of optimization. So, you're not going to change the world. Rather, you have to change your theory.

C: Scientists almost always have to idealize ...

S: Not necessarily. Not when it's going to lead you in a lot of bad predictions. If my job in the world were to be an opponent – which it's not – of neo-classical economics, I'd rub their nose in the fact that they are not able to predict anything correctly. Everything is different from what they say it is. So why should we take seriously their ideas? And they would have to respond to that.

C: The subject of economics, namely the economy, seems to be very similar to the subject of the meteorologist, namely the weather. Would it be better to acknowledge that we cannot predict very well the weather two weeks in advance and consider rethinking the idea of having an axiomatic theory all together?

S: One of the things about economics today is that you need proofs of chaotic behavior, because the basic equations are chaotic. And that's not necessarily true of all the equations of neo-classical economics. The equations in those theories are not chaotic,

and that's what's wrong with the example. You might ask what the problem is in understanding the elementary processes in the weather. How could the predictions be so bad? The answer is that the theory is chaotic. There's no hope of having extended predictions from it. That's the nature of chaotic systems. One typical feature is that they are so unbelievably sensitive to initial conditions. Another example from physics is turbulence. Turbulence gets generated in the process of things occurring that are themselves unpredictable. But none of that's discussed at all in economics.

C: So, do you think that this should be the direction in which a science like economics should be going?

S: Absolutely. Take for example something like modern finance. Modern finance is just full of unbelievably complex instruments. None of those instruments are discussed at carefully in the theory in this broad neo-classical tradition.

C: There are some attempts in econophysics that try to push in this direction ...

S: Right, and that's a good idea. That is the direction to go.

C: Would this imply a shift in focus away from studying individual decision-making with the aid of simple experiments towards studying the behavior of a complex system?

S: I think there's the following correct moral idea of what you're saying. We don't really do experiments in meteorology that are very successful and useful. We think there have been experiments that are useful, but we don't feel the need for that, because it is evident that this doesn't compare to how to deal with the chaotic behavior of the systems that are approximately correct. But that is in fact the *real* problem.

C: Should economists then still depart from an axiomatic representation of human behavior?

S: In meteorology, we start with continuous equations. But those equations describe behavior of continuous fluids that are, by assumption, considered continuous for mathematical approximation. For the purposes of the weather, they can be treated as such even though we all know they're not, in a mathematical sense, continuous. So whether economists should start with equations that describe the behavior of human agents is a question of what's going to work, not necessarily about how far to go in the reduction. The approximation of continuity in that assumption is not bad, if all you look at is the study of the flow of the saturation of water and the water vapor in the atmosphere. Studying it as a continuous process, that aspect of approximation is not where the problems are. It's not those approximations that are the source of the problem, almost surely. Rather, the source is that large-scale systems that are chaotic.

C: So how important do you consider the current work in psychology and behavioral economics?

S: Social psychologists, for example, don't even know what a chaotic system is, at least most of them.

Whether the claim that you can show that economic systems are chaotic is true of an economic system is a nice question. And it is a long way from the current mathe-

5 matical investigations. It took quite sophisticated theoretical investigation to prove that the weather system is, to first approximation, chaotic. Economists should give it a try.

By the way, in this context I should remark that Edward Norton Lorenz should be regarded as the most famous meteorologist of the twentieth-century. He really proved these things most successfully. He was my fellow predictor in Guam of bombing of
10 Tokyo, he and I prepared the predictions for the winds over 10,000 feet every day. 12 h on, 12 h off, in the last months of World War II. And the largest American air base by far, for bombing Tokyo, in Guam, Saipan, and another third island. Headquarters were in Guam, when I was in the weather central for the 20th air force. We bombed Tokyo every day. People don't realize, that we killed more people in Tokyo
15 than we killed with the atomic bomb. Another year and we'd have destroyed Tokyo. Every day he and I had the responsibility to predict the weather, and particularly the upper air winds, the planes were coming in at 20,000 feet, what were going to be the dominant winds. If you predicted a B29 to come in on the wrong way, the winds were strong enough that the plane almost was stationary over Tokyo for too long, to be hit
20 by anti-aircraft. You had to bring them in a way they got out of there fast. But that is only a side note.

C: So, mathematics should be the central ingredient of economic theories?

S: Of course! Nobody is going to have a serious theory about anything that is complicated that does not use mathematics. They may think so, but they are just kidding
25 themselves. And you have people who are deceived about that. They think that they can verbalize some really complicated phenomena successfully. We have had a bunch of experimental psychologists in history who felt that way. Well, I have to say that they are wrong!

C: Are you after truth?

30 S: Yes, I'm all for truth. It's just hard to achieve – its fragmentary.